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Inconvenient truths on merger retrospective studies

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ABSTRACT

Inconvenient truths prevent econometric merger retrospective studies from substantially altering our understanding of competitive effects from horizontal mergers. Econometrics cannot definitively determine the effects of particular mergers, and if they could, econometric merger retrospectives could not provide enough evidence to ground merger assessments in data on actual merger effects rather than in economic theory and legal presumptions. If merger retrospectives are to have some prospect of recalibrating merger enforcement, they must be transformed from econometric exercises into case studies examining the details of the relevant agency's assessment, but inconvenient truths likely prevent much from being learned through even such studies.

KEYWORDS: Antitrust, Mergers, Retrospectives, Difference-in-Differences
JEL CLASSIFICATIONS: L44, K21, L13

I. INTRODUCTION

Two colleagues and I conducted a merger retrospective study in the 1980s. We examined effects from two airline mergers allowed by the Department of Transportation over objections from the Department of Justice.[1] I tried to enlist colleagues in conducting additional retrospectives with the hope of developing substantial case study evidence for the proposition that significant horizontal mergers often produce significant price increases. Such evidence was needed then because the courts were in the sway of contestability theory[2] and were rejecting merger

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1 We found that the Justice Department's predictions of anticompetitive effects proved accurate for the Northwest–Republic merger but not for the TWA–Ozark merger. Gregory J Werden, Andrew Joskow and Richard Johnson, 'The Effects of Mergers on Price and Output: Two Case Studies from the Airline Industry' (1991) 12 Manage Decis Econ 341.

2 On the theory, see William J Baumol, John C Panzar and Robert D Willig, *Contestable Markets and the Theory of Industry Structure* (Harcourt Brace Jovanovich 1982); William J Baumol, 'Contestability: An Uprising in the Theory of Industry Structure' (1982) 72 Am Econ Rev 1. On the undue embrace of the

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challenges on the basis that entry, or the even just threat of entry, would prevent significant price increases.[3]

Much has changed over the years.[4] The contestability bubble quickly burst, and the US enforcement agencies are now doing well in court.[5] Many retrospective studies in scholarly journals now indicate that significant horizontal mergers often produce significant price increases.[6] Additional studies could prove interesting, but I see little prospect that they would, as claimed by advocates, 'provide useful guidance to aid merger enforcement'.[7]

Inconvenient truths prevent econometric merger retrospectives from providing knowledge that would significantly affect the assessment of horizontal mergers by the enforcement agencies or the courts. Such studies cannot come close to definitively determining merger effects. And even if that were not so, such studies could not support a fundamental change in merger assessment by grounding the process in data on actual merger effects rather than in economic theory and legal presumptions.

If merger retrospectives are to have any prospect of recalibrating merger enforcement, they must be transformed from econometric exercises into case studies examining the details of the relevant agency's assessment. But inconvenient truths likely prevent even case study retrospectives from materially improving the merger assessment process.

II. ECONOMETRIC MERGER RETROSPECTIVE STUDIES CANNOT DEFINITIVELY DETERMINE MERGER EFFECTS

The merger retrospectives conducted today are essentially econometric exercises, most employing the 'differences-in-differences' (DiD) estimation technique.[8] DiD

theory by the courts, see Richard Schmalensee, 'Ease of Entry: Has the Concept Been Applied Too Readily' (1987) 56 Antitrust LJ 41.

3 Three important appeals court decisions singled out the power of entry or the threat of entry in rejecting merger challenges: *United States v Baker Hughes Inc* 908 F 2d 981 (DC Cir 1990); *United States v Syufy Enterprises* 903 F 2d 659 (9th Cir 1990); *United States v Waste Management Inc* 743 F 2d 976 (2d Cir 1984).

4 Change already was evident by 1990, when the Chairman of the FTC announced a plan to answer critics of its failure to challenge certain mergers—internal retrospectives of those mergers. See Remarks of Janet D Steiger, '60 Minutes with the Honorable Janet D Steiger, Chairman Federal Trade Commission' (1990) 59 Antitrust LJ 3, 10–13.

5 The most recent court decisions in favour of the agencies in merger challenges are: *St Alphonsus Medical Center–Nampa Inc and FTC v St Luke's Health System, Ltd* 778 F 3d 775 (9th Cir 2015); *ProMedica Health System, Inc v FTC* 749 F 3d 559 (6th Cir 2014); *Polypore International Inc v FTC* 686 F 3d 1208 (11th Cir 2012); *United States v Bazaarvoice Inc* 2014–1 Trade Cas (CCH) para 78,641 (ND Cal 2014); *FTC v OSF Healthcare System* 852 F Supp 2d 1069 (ND Ill 2012); *United States v H & R Block Inc* 833 F Supp 2d 36 (DDC 2011).

6 A useful compilation is provided by John Kwoka, *Mergers, Merger Control, and Remedies* (MIT Press 2015).

7 Orley Ashenfelter, Daniel Hosken and Matthew Weinberg, 'Generating Evidence to Guide Merger Enforcement' (2009) 5(1) Comp Policy Intl 57, 72.

8 Useful introductions to DiD estimation are Alberto Abadie, 'Difference-in-Difference Estimators' in Steven N Durlauf and Lawrence E Blume (eds), *The New Palgrave Dictionary of Economics*, vol 2 (2nd edn, Palgrave MacMillan 2008) 490; Graeme Hunter, Gregory A Leonard and G Steven Olley, 'Merger Retrospective Studies: A Review' (2008) 23(1) Antitrust 34; and Kwoka (n 6) 57–70. DiD merger retrospectives are touted as the best way to inform merger policy by an influential article on empirical approaches to economics. See Joshua D Angrist and Jörn-Steffen Pischke, 'The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con Out of Econometrics' (2010) 24(2) J Econ Perspectives 3, 20–22.

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estimation is a standard method for evaluating programmes or market interventions, such as an increase in the minimum wage.[9] It builds on old insights into estimating the effect of a treatment, like a new drug being tested for efficacy and side effects. DiD estimation compares the change an indicator for a treatment group to the change in that indicator for a control group. The technique, generally, is applied to 'panel data', which are repeated observations of multiple subjects.

In a merger retrospective, the treatment is the merger, and the indicator normally is a measure of price. The typical merger retrospective compares changes in prices for a treatment group affected by the merger to changes in prices for a control group unaffected by the merger. The treatment group normally consists of products that are sold by the merging firms and as to which the competitive effects of the merger are being investigated. The competitive effects theories in most horizontal merger cases, however, predict price effects for rivals as well. Indeed, uniform market-wide price effects are predicted by most coordinated effects theories and some unilateral effects theories. Hence, the treatment group could be all products in the relevant market.

DiD estimation is widely applied by economists to 'natural experiments', like mergers, which lack the randomization and careful control of laboratory experiments.[10] Researchers applying DiD estimation to natural experiments argue that the conditions under study 'approximate[] the conditions of a controlled experiment', but critics often are sceptical.[11] Randomized, controlled experiments remain the gold standard even in economics, and many have been performed to inform economic policy debates.[12] Mergers, however, offer only non-randomized, uncontrolled experiments.

For an otherwise well-conducted merger retrospective employing DiD estimation, the 'validity' of its estimate 'turns transparently on the quality of the control group'.[13] To serve as a proper control, the unaffected prices must be influenced by exactly the same supply and demand forces that influence the affected prices.[14] Although this

9 On these methods generally, see Joshua D Angrist and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton University Press 2008) 221–47; Guido W Imbens and Jeffrey M Wooldridge, 'Recent Developments in the Econometrics of Program Evaluation' (2009) 47 J Econ Lit 5; Bruce D Meyer, 'Natural and Quasi-Experiments in Economics' (1995) 13 J Bus Econ Stat 151.

10 Researchers typically assume the 'unconfoundedness' of a natural experiment. See Imbens and Wooldridge (n 9) 23, 25. With merger retrospectives, this means that no unobserved determinant of prices was part of the rationale for merger. For example, it means that the acquiring firm did not act on information unavailable to the econometrician indicating that the acquired firm was about to enjoy higher prices. Violation of the unconfoundedness assumption causes the estimate of the merger effect to be biased. See Daniel L Rubinfeld, 'Econometric Issues in Antitrust Analysis' (2010) 166 J Inst Theor Econ 62, 74.

11 See Charles F Manski, *Public Policy in an Uncertain World: Analysis and Decisions* (Harvard University Press 2013) 61–63.

12 ibid 66–70.

13 Angrist and Pischke (n 8) 21.

14 'The difficulty of the difference-in-difference approach is in identifying a good control product (or group of products) for the products produced by the merging firms. In an antitrust setting, for example, there is often a tension between finding products that are in different geographic markets and therefore not affected by the transaction, while truly facing similar demand and cost conditions.' Ashenfelter and others (n 7) 64 (footnote omitted). A study devoting special attention to the control group is Phillippe Chroné

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might suggest looking for control products within the relevant markets, the prices of products in the relevant market are likely to be affected by the merger, so using them biases the estimate of merger effect.[15] With local markets, locations not affected by the merger generally provide a suitable control group.[16] Finding suitable controls is more challenging when relevant markets are very broad in geographic scope.[17]

DiD estimation with no supply or demand variables in the regression yields an unbiased estimate of the merger's price effect only if the time trend of prices is the same for the treatment and control groups.[18] Both this point and DiD estimation in general are clarified in the following example involving the hypothetical merger of the only two gas stations in a small US town:

Suppose the price for regular gasoline increased from an average of $3.09 per gallon over six months before the merger to $3.59 per gallon over six months after the merger.[19] An uncontrolled, and hence unreliable, estimate of the merger's effect is that it increased price 50 cents per gallon. A much better estimate is obtained using prices in the next town as a control, and suppose they increased from an average of $3.29 before the merger to $3.49 after the merger, a difference of 20 cents per gallon. Differencing the differences yields an estimated merger effect of 30 cents per gallon. We 'identify' the merger effect through the assumption that supply and demand forces have a 'fixed effect' on the difference in prices between the two towns. That assumption implies that a time trend accounts for a price increase of 20 cents per gallon.

DiD regressions in merger retrospectives often include supply and demand variables, termed 'covariates', rather than assume fixed effects. A DiD estimation using covariates is unbiased if prices in the treatment and control groups respond in the same way to the covariates.[20] In estimating the price effect of the foregoing gas station merger, wholesale gasoline prices could be included in a DiD regression under

and Laurent Linnemer, 'A Treatment Effect Method for Merger Analysis with an Application to Parking Price in Paris' (2012) 55 J Indus Econ 631.

15 For one important unilateral effects model, advocates of merger retrospectives have argued that the bias leads to underestimation of the price increases from a merger. Ashenfelter and others (n 7) 65. That is not necessarily correct if the merger generated efficiencies. The impact on rivals' prices of reductions in the merged firm's marginal costs, including whether rivals' price increase or decrease, depends on idiosyncratic properties of particular demand functions. See Gregory J Werden, Luke M Froeb and Steven Tschantz, 'The Effects of Merger Efficiencies on Consumers of Differentiated Products' (2005) 1 Eur Comp J 245.

16 The estimated merger effects can vary substantially depending on which control locations are selected, as illustrated by Christopher T Taylor and Daniel S Hosken, 'The Economic Effects of the Marathon-Ashland Joint Venture: The Importance of Industry Shocks and Vertical Market Structure' (2007) 55 J Indus Econ 419.

17 A leading study of consumer products mergers used private labels as the controls. Orley Ashenfelter and Daniel Hosken, 'The Effect of Mergers on Consumer Prices: Evidence from Five Mergers on the Enforcement Margin' (2010) 53 JL & Econ 417. Critics doubted the claim that private label products are only weak substitutes for major brand products. Stephen W Davies and Peter L Ormosi, 'A Comparative Assessment of Methodologies Used to Evaluate Competition Policy' (2012) 8 J Comp L Econ 769, 782.

18 See Angrist and Pischke (n 9) 228–31; Ashenfelter and others (n 7) 64.

19 A period around the time of the merger is typically omitted because it is unclear when the impact of the merger was first felt or when it was fully felt. See Ashenfelter and others (n 7) 68.

20 See Alberto Abadie, 'Semiparametric Difference-in-Differences Estimators' (2005) 72 Rev Econ Stud 1, 1–4; Imbens and Wooldridge (n 9) 67–68; Meyer (n 9) 156.

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the plausible assumption that the pass-on rate of wholesale price changes is the same in the control and treatment towns. In many merger retrospectives, however, there is little reason to assume that all covariates affect the treatment and control groups to the same degree.

Economists John Simpson and David Schmidt of the US Federal Trade Commission question the validity of assumptions underlying DiD estimation in antitrust.[21] Using simple supply and demand graphs, they show that demand and cost shocks can have different price effects in markets that differ with respect to the elasticity of supply or demand. If the treatment and control groups differ significantly in such ways, DiD estimates are biased. Simpson and Schmidt also show how a merger retrospective yields a biased estimate if the degree of competition differs between treatment and control markets. The reason is that monopoly prices respond less to cost and demand shifts than do competitive prices. Finally, they show that differences in the degree of substitutability among differentiated products between treatment and control markets can introduce a substantial bias, potentially reversing the sign of the estimate.[22]

As with most econometric work, merger retrospectives present myriad choices: data sources, price measures, time-periods, and statistical methods. Few results are so robust that no choice matters, and different researchers are apt to make choices different enough to produce substantially different estimates of merger effects. A case in point is a study that found Atlantic Richfield Company's long-term lease and conversion of Thrifty gasoline stations in California led to significant prices increases.[23] Later researchers using a different data source found that the effect was trivial,[24] and the original researcher confirmed that the two data sources do yield very different estimates.[25]

After the General Accounting Office (GAO) issued a report on the wholesale price effects of mergers in the US petroleum industry,[26] an FTC staff report questioned many choices the GAO had made and showed that estimates of merger effects were sensitive to them.[27] The only large anticompetitive effect found by the GAO

21 John Simpson and David Schmidt, 'Difference-in-Differences Analysis in Antitrust: A Cautionary Note' (2008) 75 Antitrust LJ 623.

22 Simpson and Schmidt do not make the comparable point that bias is introduced if demand curvature differs significantly between treatment and control markets. The rate at which costs changes are passed through to prices is determined by the curvature of demand. See Werden and others (n 15).

23 Justine S Hastings, 'Vertical Relationships and Competition in Retail Gasoline Markets: Empirical Evidence from Contract Changes in Southern California' (2004) 94 Am Econ Rev 317.

24 Christopher Taylor, Nicholas Kreisle and Paul Zimmerman, 'Vertical Relationships and Competition in Retail Gasoline Markets: Comment' (2010) 100 Am Econ Rev 1269.

25 Justine S Hastings, 'Vertical Relationships and Competition in Retail Gasoline Markets: Empirical Evidence from Contract Changes in Southern California: Reply' (2010) 100 Am Econ Rev 1277.

26 US GAO, Energy Markets: Effects of Mergers and Market Concentration in the US Petroleum Industry (May 2004) <www.gao.gov/new.items/d0496.pdf> accessed 13 June 2015. An abbreviated report on the research was published as John A Karikari and others, 'The Impact of Mergers in the US Petroleum Industry on Wholesale Gasoline Prices' (2007) 25 Contemp Econ Policy 46.

27 FTC Staff Technical Report, Robustness of the Results in the GAO's 2004 Report Concerning Price Effects of Mergers and Concentration Changes in the Petroleum Industry (21 December 2004) <http://www.ftc.gov/system/files/documents/reports/ftc-staff-technical-report-robustness-results-gaos-2004-report-concerning-price-effects-mergers/ftcstafftechnicalreport122104.pdf> accessed 13 June 2015. The FTC organized a conference to discuss the GAO report, and the participants' views are summarized by

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was an increase in the price of branded gasoline by 6.9 cents per gallon from the 1997 purchase by Tosco of a Unocal refinery in California. FTC economists later estimated that the acquisition led to a significant *decrease* in the price of branded gasoline.[28]

A study of the merger of Northwest and Delta airlines found that the estimated effect on fares depends a great deal on the choice of the control group.[29] Using standard DiD methodology with several different control groups, the study estimates average fare increases between 0 per cent and 6 per cent. Using a method that identifies the single best match control for each route affected by the merger, the study finds a 1 per cent fare increase.

Furthermore, a merger retrospective could substantially mislead if merger effects are complicated. For example, significant consumer benefits could flow from increased quality or a new product made possible by a merger, but standard methods would not detect this merger effect. New products are normally omitted, and while quality changes might be accounted for, that is difficult and unlikely to be attempted. If customers got a much better product at a slightly higher price, standard DiD estimation could erroneously indicate that the merger's effect was to increase price anticompetitively.

A merger retrospective also is likely to estimate only short-term merger effects, for example, effects within the first year or two, yet long-term merger effects could be quite different. The process of integrating the merging firms could take years, and fully implementing new strategies could take even longer. Reponses by rivals could take longer still. This could explain why a few merger retrospectives have found long-term merger effects that differed materially from short-term effects.[30] Simply extending the period of post-merger observation, however, is not a solution. Subsequent mergers often limit the uncontaminated period of observation, and the longer the time-period of post-merger observation, the less credible the identification of merger effects becomes. It is not credible that a change in pricing five years after a merger is from the merger, and it is doubtful that both the control and treatment groups can be equally affected by same forces over a long period of time.

An econometric estimate of a merger effect also is subject to statistical sampling error. The estimate is not a number, but rather a probability distribution, possibly

Luke M Froeb and others, 'Economics at the FTC: Cases and Research, with a Focus on Petroleum' (2005) 27 Rev Indus Org 223, 233–37.

28 Daniel Hosken, Louis Silvia and Christopher Taylor, 'Does Concentration Matter? Measurement of Petroleum Merger Price Effects' (2011) 100 Am Econ Rev (Papers & Proceedings) 45.

29 Aditi Mehta and Nathan Miller, 'Choosing the Appropriate Control Group in Merger Evaluations' in *The Pros and Cons of Merger Control* (Swedish Competition Authority 2012) 189.

30 Several retrospective found that the merger of Northwest and Republic airlines resulted in substantial fare increases. See eg Severin Borenstein, 'Airline Mergers, Airport Dominance, and Market Power' (1990) 80 Am Econ Rev 400; Werden and others (n 1). However, a follow-up study found a much smaller long-term effect on fares. Steven A Morrison, 'Airline Mergers: A Longer View' (1996) 30 J Transport Econ Policy 237. A study of two petroleum refinery acquisitions in California found substantially smaller price effects in the second year after the acquisition than in the first year. Hosken and others (n 28). Finally, a study of Italian bank mergers found significant anticompetitive effects on interest rates paid to depositors for the first three years but the reverse for the next three years. Dario Focarelli and Fabio Panetta, 'Are Mergers Beneficial to Consumers? Evidence from the Italian Market for Bank Deposits' (2003) 93 Am Econ Rev 1152.

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with a large variance.[31] Moreover, the reported variance of the estimate reflects only 'part of the relevant uncertainty' in that it 'does not capture the uncertainty in the estimates due to *ad hoc* choices made in the model specification'.[32] Policy-relevant empirical research such as merger retrospectives suffers from what Charles Manski termed 'incredible certitude'.[33] Academic researchers downplay the true uncertainty of their findings, and that uncertainty disappears altogether when the research is brought into the policy domain. Illustrative is the recent book on merger policy by John Kwoka; he criticizes enforcement on the basis of findings from merger retrospectives with no accounting for the uncertainty in those findings.[34]

The point of the forgoing is not that merger retrospectives are useless. It is that that they are not nearly as useful as might be supposed. An inconvenient truth is that econometric merger retrospectives cannot come close to definitively determining actual merger effects.

III. ESTIMATED MERGER EFFECTS FROM ECONOMETRIC STUDIES LIKELY COULD CONTRIBUTE LITTLE TO IMPROVE MERGER ENFORCEMENT

Those proposing additional econometric merger retrospective studies aspire not merely to demonstrate the potential of horizontal mergers to increase prices. They propose using retrospectives to recalibrate merger enforcement. Two strident critics of the Bush Administration's enforcement record argued in 2008:

> The most compelling way to evaluate the accuracy of merger enforcement policy would be through merger retrospectives—detailed studies evaluating the actual effects of consummated mergers on market prices, product variety, or innovation. The most revealing mergers to study in depth are those that went forward despite presenting serious antitrust concerns. Armed with a large number of such studies, one could, in principle, identify the conditions under which horizontal mergers do, and do not, harm consumers.[35]

But inconvenient truths preclude nearly every way of using econometric merger retrospectives to recalibrate merger enforcement.

Estimates of merger effects from retrospective studies could not be used in a straightforward adjustment of market share thresholds for the simple reason that market shares are determinative of neither agency assessments nor actual merger effects. Market shares remain important in enforcement, but the agencies and courts

31 Econometric research suggests that many published DiD studies in economics grossly understate the true variance of their estimates. See Marianne Bertrand, Esther Duflo and Sendhil Mullainathan, 'How Much Should We Trust Difference-in-Differences Estimates' (2004) 119 QJ Econ 249; Stephen G Donald and Kevin Lang, 'Inference with Difference-in-Differences and Other Panel Data' (2007) 89 Rev Econ Stat 221; Imbens and Wooldridge (n 9) 69.

32 Guido Imbens, 'Book Review Feature: *Public Policy in an Uncertain World*' (2013) 123 Econ J F401, F402–03.

33 Charles F Manski, 'Policy Analysis with Incredible Certitude' (2011) 121 Econ J F261.

34 Kwoka (n 6).

35 Jonathan B Baker and Carl Shapiro, 'Detecting and Reversing the Decline in Horizontal Merger Enforcement' (2008) 22(3) Antitrust 29, 29.

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also employ more sophisticated and subtle analysis and find that given market shares have different implications for likely merger effects in different settings.[36]

Even if market share thresholds were determinative of both predicted and actual merger effects, merely estimating merger effects would not be useful in recalibrating the thresholds because the delineation of the relevant market would be the dominant issue in merger assessments, as once was true.[37] Because econometric merger retrospectives do not address market delineation,[38] they can shed only dim light on the actual market shares associated with whatever actual merger effects are found.

Nor could estimates of merger effects from retrospective studies be used as data in a multivariate analysis relating merger effects to an array of variables reflecting potentially relevant factors. Econometric merger retrospectives do not supply these variables, nor can merger assessment be reduced to a simple linear function of objectively measureable variables. Moreover, even with vastly more estimates of merger effects than we currently have, the observation of retrospective proponents from a few years ago would remain valid: '[T]he number of merger retrospectives [still would be] far too small to make any inference on which market characteristics are correlated with price effects'.[39] Merger assessment is so heavily fact dependant that every case—or at least every close case—is unique, so the available data cannot trace out a general rule.

Estimates of merger effects from econometric retrospective studies also cannot be used to answer critics who argue that merger enforcement is too strict. Merger retrospectives cannot shed even the dimmest light on the effects of mergers never proposed, abandoned because of agency opposition, or enjoined prior to consummation. Nor can merger retrospectives be used to test the specific allegations in vast majority of agency complaints that are resolved either by consent or by a court decision in the government's favor.[40]

36 Many professionals who work in the merger area prefer to downplay market delineation and market shares and instead move more directly to assessing likely competitive effects. In merger litigation today, much of the evidence and argument, especially from the merging parties, is devoted to likely competitive effects.

37 A quarter century ago, Robert Pitofsky wrote that 'the legality of mergers depends on the market shares of the merging parties and, therefore, on the question of what is the proper market. . . . Knowledgeable antitrust practitioners have long known that the most important single issue in most enforcement actions-because so much depends on it-is market definition'. Robert Pitofsky, 'New Definitions of Relevant Market and the Assault on Antitrust' (1991) 90 Columbia L Rev 1805, 1807.

38 Estimated price increases from hospital mergers were argued by retrospectives proponents to 'plainly show that the expansive geographic markets' advocated by defendants on the basis of patient migration 'are quite likely to understate the likelihood of harm from hospital mergers'. Orley Ashenfelter and others, 'Retrospective Analysis of Hospitals' (2011) 18 Intl J Econ Bus 5, 14. But the US enforcement agencies have not made the mistake of delineating expansive geographic markets on the basis of patient migration, and scholarly literature long ago explained why it was wrong to do so. See Gregory J Werden, 'The Limited Relevance of Patient Migration Data in Market Delineation for Hospital Merger Cases' (1989) 8 J Health Econ 363. Moreover, any inference about the relevant market drawn from estimated merger effects must be based on beliefs about the relationship between market shares and merger effects, yet the purpose of merger retrospectives is to supplant beliefs with hard data.

39 Ashenfelter and others (n 7) 69.

40 Moreover, merger retrospectives using DiD estimation cannot generate useful evidence on coordinated effects, if, as modern economics posits, a merger merely increases the probability of a still unlikely coordination event. Suppose the probability that coordination in any month was 0.2 per cent before a merger

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Neither do mergers challenged after they are consummated provide an opportunity to test the efficacy of the assessment process for proposed mergers. When the actual effects of a consummated merger can be observed reliably, the assessment process for proposed mergers is unlikely to be employed.[41] And when the actual effects of a consummated merger cannot be observed reliably, because the merged firm was under scrutiny and could have avoided actions that would have undermined its litigation position,[42] studying the period between consummation and challenge is pointless.

If estimates of merger effects from retrospective studies are to play a role in recalibrating enforcement, the estimates will have to be for mergers that went unchallenged because the merger enforcement agencies judged that they were not likely to lessen competition substantially. Of course, the vast majority of such mergers are of little interest because they raise no competitive concerns.[43] The mergers of principal interest are those not challenged despite raising significant competitive concerns.[44]

Retrospectives advocates have argued that: 'By focusing on mergers that were on the enforcement margin, researchers can begin to develop empirical evidence on which types of mergers are likely to be problematic and can provide useful guidance to aid merger enforcement.'[45] But these advocates have not explained what guidance they anticipate or how retrospectives provide it. Presumably, they intend something simple; for example, if merger retrospectives often estimate adverse merger effects, the guidance would be to

and that a merger increases that probability to 1 per cent. The probability of coordination at least once a decade was about 21 per cent before the merger and about 70 per cent after it, so the merger produces a significant coordinated effect. Now suppose a retrospective study uses two years of monthly data before and after the merger. The probability that coordination will occur after, but not before, the merger is just 20 per cent. Even if the occurrence of coordination could be observed without error, the study would have little power to determine whether the merger actually increased the probability of coordination.

41 In 1957, the Supreme Court held that du Pont's 1917–19 acquisition of shares in General Motors was unlawful on the basis of effects observed over the many years since the acquisition. *United States v EI du Pont de Nemours & Co* 353 US 586 (1957). The first published merger retrospective study was associated with the FTC's 1981 challenge of two consummated acquisitions in the microfilm business after substantial prices increased had occurred. David M Barton and Roger Sherman, 'The Price and Profit Effects of Horizontal Mergers: A Case Study' (1984) 38 J Indus Econ 165. The complaint and consent order are at *Xidex Corp*, 102 FTC 1 (1983).

42 In *United States v Bazaarvoice, Inc* 2014–1 Trade Cas (CCH) para 78,641, 129,064-65 (ND Cal 2014), the court gave little weight to post-consummation pricing evidence because it '*could arguably* be subject to manipulation' (emphasis in original). The court relied on *United States v General Dynamics Corp* 415 US 486, 505-06 (1974) ('If a demonstration that no anticompetitive effects had occurred at the time of trial or of judgment constituted a permissible defense to a s 7 divestiture suit, violators could stave off such actions merely by refraining from aggressive or anticompetitive behavior when such a suit was threatened or pending.'), and *Chicago Bridge & Iron Co v FTC* 534 F 3d 410, 435 (5th Cir 2008) (holding that evidence proffered by the defendant was not probative because it '*could arguably* be subject to possible manipulation' (emphasis in original)).

43 Examining a very large number of mergers raising no competition concerns could be informative of the background noise in the estimates of merger effects using DiD estimation or alternative methods. If substantial adverse merger effects were estimated for a significant proportion of mergers raising no competitive concerns, that finding would cast doubt on estimates for mergers raising concerns.

44 Such mergers were the subject of many retrospectives. See eg Ashenfelter and Hosken (n 17). A retrospectives survey termed such mergers 'particularly informative" and "the most relevant population for policy purposes'. Kwoka (n 6) 86, 158.

45 Ashenfelter and others (n 7) 72.

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tighten enforcement. But such guidance is too vague to be of much help,[46] and merger retrospectives cannot easily provide even such vague guidance.

As Dennis Carlton explained, adverse effects from an unchallenged merger do imply error on the enforcement agency's part.[47] Merger enforcement is inherently subject to false negatives and false positives.[48] Estimating merger effects for unchallenged mergers raising competitive concerns could shed light on the actual rate of false negatives, but the optimal rate is unknown. Merger retrospectives alone can indicate that the rate of false negatives is too high only if it is found to be close to 100 per cent.

Finally, it must be noted that merger retrospectives too often engender puzzlement rather than enlightenment. A review of hospital merger retrospectives observed that a 'common empirical finding' has been that the price effects of a given merger 'differed substantially across the different commercial insurers'.[49] One study reported that the merged hospitals raised prices for two of the insurers with which they contracted, kept prices the same for one insurer, and substantially reduced prices for a fourth insurer.[50] A comment on the study observed that methodological infirmities could account for such findings,[51] but if the reported merger effects were taken seriously, the disturbing implication evidently would be that competition works in unfathomable ways.

Similarly, a retrospective of the Maytag–Whirlpool merger found that it led to a substantial price increases for clothes dryers, but it did not lead to price increases for clothes washers.[52] This is odd because the market share distributions for washer and dryers were not materially different[53] nor were supply or demand conditions.[54] The odd finding could be the result of methodological problems, but if the study were to be taken at face

46 Retrospectives advocate John Kwoka argues that existing studies demonstrate that US merger enforcement has been too lax, yet he recognizes that 'filing more complaints' is not a satisfactory policy prescription. Kwoka (n 6) 159.

47 Dennis W Carlton, 'The Need to Measure the Effect of Merger Policy and How to Do It' (2008) 22(3) Antitrust 39, 40; Dennis W Carlton, 'Why We Need to Measure the Effect of Merger Policy and How to Do It' (2009) 5(1) Comp Policy Intl 77, 79–80 (Carlton 2). See Thomas O Barnett, Assistant Attorney General, Antitrust Division, US Department of Justice, Current Issues in Merger Enforcement: Thoughts on Theory, Litigation Practice, and Retrospectives, Lewis Bernstein Memorial Lecture, Washington, DC, 26 June 2008 ('Appropriate calibration of merger enforcement policy involves (in addition to trying to improve our analytical tools to reduce the risk of error) striking the appropriate balance between Type I and Type II errors. Even in the most perfectly calibrated system, any given decision might turn out to have been erroneous in retrospect notwithstanding that it was correct based on the information available at the time.') <http://www.justice.gov/atr/public/speeches/234537.pdf> accessed 13 June 2015.

48 This is implicit in the 'reasonable probability' test applied to mergers in the USA. s 7 of the Clayton Act prohibits mergers the effect of which 'may be substantially to lessen competition', and the Supreme Court held that a proposed merger violates s 7 'if there is a reasonable probability that the merger will substantially lessen competition'. *Brown Shoe Co v United States* 370 US 294, 325 (1962).

49 Deborah Haas-Wilson and Michael Vita, 'Mergers Between Competing Hospitals: Lessons from Retrospective Analyses' (2011) 18 Intl J of the Econ of Bus 1, 3.

50 Aileen Thompson, 'The Effect of Hospital Mergers on Inpatient Prices: A Case Study of the New Hanover–Cape Fear Transaction' (2011) 18 Intl J Econ Bus 91.

51 Gregory K Leonard and G Steven Olley, 'What Can Be Learned about the Competitive Effects of Mergers from "Natural Experiments"?' (2011) 18 Intl J Econ Bus 103.

52 Orley C Ashenfelter, Daniel S Hosken and Matthew C Weinberg, 'The Price Effects of a Large Merger of Manufacturers: A Case Study of Maytag-Whirlpool' (2013) 5 Am Econ Rev: Econ Policy 239.

53 ibid 246.

54 To account for the difference, the study pointed to fact that washers were in the midst of a technology change. ibid 255.

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value, an unknown force must have caused the merger to have dramatically different effects in nearly identical markets. Identifying such a force, if indeed it exists, would be useful, but econometric merger retrospectives will not do that.

Merger retrospectives are not nearly as useful as advocates suggest. Econometric merger retrospectives will not be able to support grounding the merger assessment process in data on actual merger effects rather than in economic theory and legal presumptions.

IV. MAKING MERGER RETROSPECTIVES MORE USEFUL

More useful merger retrospectives would combine econometrics with the case study approach once *de rigueur*,[55] seeking to square estimated merger effects with the details of relevant agency's assessment. As argued by Dennis Carlton: 'Only in that way—by combining a record of what tools were used and what conclusions were drawn from each tool with a study of observed outcomes from mergers—can systematic evidence be collected on the efficacy of various methods used in merger review.'[56] And although Carlton proposed to use merger retrospectives only to test analytic tools, they could be used to test the entire merger assessment process.

If a merger under study did not get a close look by the agency, its retrospective study could ask not only whether it proved significantly anticompetitive, but also, if so, whether the agency missed a signpost that should have directed it to investigate. If the study uncovers no premerger inkling of the anticompetitive effects to come, it merely reconfirms the error inherent in the assessment process. To provide guidance, the study would have to identify special circumstances that caused a seemingly benign merger to prove anticompetitive, and the study would then have to spawn a workable method for flagging future mergers presenting comparable circumstances. Neither achievement seems likely.

For a merger allowed by a court over an enforcement agency's objections, a retrospective study would be useful only if it pinpointed an error and convincingly explained that it was an analytic error rather than a misjudgement or failure of proof. Because the enforcement agency challenging the merger has a burden of proof, some anticompetitive mergers will survive court challenges. Furthermore, merely identifying a court's analytic error does not prevent reoccurrence.

The vast majority of retrospectives, most likely, will study mergers that were thoroughly investigated yet not challenged. In addition to estimating actual merger effects, studies of such mergers could ask whether post-merger competition conformed to the agency's prior predictions, and if not, what specific aspects of the agency's assessment were inaccurate. Identifying a source of systematic error in agency merger

55 See eg Morris A Adelman, *A&P: A Study in Price-Cost Behavior and Public Policy* (Harvard University Press 1959); William N Nicholls, *Price Policies in the Cigarette Industry: A Study of 'Concerted Action' and Its Social Control, 1911–50* (Vanderbilt University Press 1951); Simon N Whitney, *Antitrust Policies: American Experience in Twenty Industries* vol 2 (Twentieth Century Fund 1958) (reviewing 10 famous antitrust cases and how they affected competition). Much the same sort of work was done decades later when the FTC commissioned academics to review the impact of its vertical restraints enforcement. See Ronald N Lafferty, Robert H Lande and John B Kirkwood (eds), *Impact Evaluations of Federal Trade Commission Vertical Restraint Cases* (Federal Trade Commission 1984).

56 Dennis W Carlton, 'Revising the Horizontal Merger Guidelines' (2010) 6 J Comp L Econ 619, 651.

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assessment could lead to a significant recalibration of enforcement, but inconvenient truths make identifying sources of error difficult.[57]

When a retrospective study convincingly finds that what happened after a merger was far from what the agency predicted, the error must be due to some combination of faulty facts, erroneous economics, bad breaks, and random variation. To illustrate, posit a merger found to have resulted in significant anticompetitive effects although the responsible agency predicted no adverse effects on the basis that entry would prevent them.

The agency could have found faulty facts by relying on false representations made by a potential entrant. The agency could have relied on erroneous economics by misapprehending the impact of the merger on the profitability of entry. The agency could have gotten a bad break by failing to anticipate that critical technology needed for entry would become unavailable for licence. And random variation could have affected each aspect of the agency's entry analysis. For example, a firm planning to enter could have been forced by financial setbacks to scrap its plans.

Merger investigations are all about finding facts, and factual error can alter the enforcement decision. A retrospective study finding a major factual error could identify ways to improve fact-finding. For example, the agency might be more sceptical about statements made by industry participants or more thorough in canvassing customers. It seems unlikely, however, that merger retrospectives would yield constructive suggestions for improving fact-finding.

When a major prediction error is attributed to mistaken analysis, the next task is identifying the mistake made, which could be in the selection of tools, in the application of the tools selected, or in the interpretation of results. Every opportunity for a mistake could be examined.

Consider a differentiated consumer product merger assessed with a Bertrand merger simulation calibrated by estimating demand elasticities from scanner data, and assume the agency's work was free of technical error. A retrospective study could consider whether competition in the industry works differently than the Bertrand model predicts. In that event, the study could attempt to identify general industry features indicating the Bertrand model would be a poor guide in a merger assessment.[58]

57 In conducting retrospective studies of past mergers, a problem is that agencies may not have generated or retained sufficiently detailed documentation of their assessments. To remedy the problem, Dennis Carlton proposed that: 'For each merger that the agencies review closely . . . , they should record which analytical tools they employed and what predictions they reached with each tool. Then, for those mergers that are consummated, the antitrust agencies should undertake retrospective reviews of actual marketplace outcomes in comparison with those predictions.' Carlton (n 56) 651. The task, however, is not as straightforward as he suggests. As just one example, analytical tools might have accurately predicted significant adverse effects from a merger, yet the agency decided not to rely on them. In such an event, a retrospective should evaluate not the tools, but rather the agency's decision making on when to rely on them.

58 A model used to simulate merger effects could itself be faulty, but retrospective studies cannot prove that it is. No model fits every market, and perhaps no model fits even 20 per cent of markets, so a model's failure to predict accurately, even over many cases, cannot prove that it does not predict accurately for some class of cases.

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The study could revisit the modelling of consumer demand, in particular, the use of particular econometric methods or any modelling choices that might seem unwise in hindsight. Any choice that matters is a potential source of significant error, but it seems unlikely that merger retrospectives would teach much about how to make the right choices in future work, in part because it is difficult to generalize from specific cases.

Data-based approaches also are used in merger assessments,[59] and their utility could be examined in merger retrospectives. When such methods are used, quantitative predictions of merger effects are produced, and such predictions could be squared with estimated merger effects. In particular, merger retrospectives could be used to determine whether DiD estimation exploiting natural experiments accurately predicts merger effects. When a substantial prediction error is found, the sources of error could be examined. If a merger assessment relied on a natural experiment other than a merger, a critical issue is whether it was a suitable experiment from which to predict likely merger effects.

V. WHO SHOULD CONDUCT THE MERGER RETROSPECTIVES?

Ample reasons are set out above for agencies to decide against devoting resources to merger retrospectives.[60] Yet, as explained by Dennis Carlton, agencies 'are uniquely well positioned' to conduct retrospectives of the mergers they assessed.[61] Retrospectives offering some prospect of refining the merger assessment process require access to the responsible agencies' confidential fact-finding, data, analysis, and conclusions, and the agencies uniquely have access.[62]

An objection to having agencies do retrospectives on mergers under their purview is that such studies would have the appearance of bias. But that is a concern only when agencies seek to influence courts, legislatures, or public opinion. An appearance of bias cannot undermine any impact merger retrospectives have in refining an agency's merger-assessment skills. Moreover, agencies could mitigate the appearance of bias: staff economists could do studies independently of agency leadership and subject the studies to peer review through the normal process of publication in

59 Two cases in which such tools were employed in litigation are *FTC v Whole Foods Market Inc* 502 F Supp 2d 1 (DDC 2007), and *FTC v Staples Inc* 970 F Supp 1066 (DDC 1997). On the use of such tools in the former case, see Jonathan B Baker, 'Econometric Analysis in *FTC v Staples*' (1999) 18 J Public Policy Mark 11. On the latter, see ABA Section of Antitrust Law, *Econometrics* (2nd edn, American Bar Association 2014) 277–81. Because the FTC's challenge was successful in both cases, neither is a candidate for a merger retrospective.

60 In addition, small or inexperienced competition agencies might lack the resources or expertise to perform merger retrospective studies.

61 Carlton (n 56) 651. Some enforcement agencies, including the Federal Trade Commission, are authorized to conduct studies and to use compulsory process as needed in conducting them.

62 Academics lacking access to agency files could attempt to reverse engineer agency assessments. Essentially, that is what was done by Matthew C Weinberg and Daniel Hosken, 'Evidence on the Accuracy of Merger Simulations' (2013) 95 Rev Econ Stat 1584, and by Matthew C Weinberg, 'More Evidence of the Performance of Merger Simulations' (2011) 101 Am Econ Rev (Papers & Proceedings) 51. But the apparent prediction error in such a study could be entirely due the academic's failure to replicate agency's actual assessment. The academic could select a tool for predicting effects that the agency had wisely determined to be inappropriate. The academic also could make poor choices in applying a tool that the agency did not make because of its superior information on the industry.

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scholarly journals.[63] Staff economists at the FTC have been wholly or partly responsible for many of the merger retrospectives published in scholarly journals.

Furthermore, bias should not be a concern only when agencies review their own work. Outside researchers directing their energies into merger retrospectives care about how they come out, and most have an anti-enforcement bias. The fact that badly flawed studies criticizing antitrust enforcement have been published by major economics journals[64] suggests that the journals did too little to mitigate this bias or that their editors also were biased.

Competition agencies could contract with outside researchers to do retrospectives,[65] but agency contractors could be viewed as no less biased than the agency people who select them. Moreover, agencies might balk at paying outsiders at rates 10 times what staffers are paid. Highly qualified and unbiased researchers also might be difficult to enlist. Untenured academics might prefer other projects with greater potential to boost tenure prospects. Academics with solid scholarly reputations might be interested only in advancing personal agendas on methodology or policy.

VI. CONCLUSIONS

Inconvenient truths prevent econometric merger retrospectives from definitively determining merger effects and from supporting substantial change in merger assessment. To have any prospect of materially recalibrating merger enforcement, merger retrospectives must be transformed into case studies that square estimated merger effects with the details of relevant agency's assessment. Inconvenient truths, however, likely prevent even this sort of retrospective study from significantly improving the merger assessment process.

Creating a substantial body of merger retrospectives findings also could have a downside. It would invite the argument that empirical findings of actual merger effects should be given controlling weight in formulating merger policy. And it could

63 Many other arrangements could be considered. For example, two competition agencies could review each other's work. But such elaborate efforts to avoid the appearance of bias likely would be more trouble than they are worth. Moreover, ill will would be generated if either agency found fault with the other's work, and, recognizing this, outsiders might find that such an arrangement does little to mitigate bias.

64 The *Journal of Law & Economics* published a stock market event study purporting to show that challenged horizontal mergers were not anticompetitive. B Espen Eckbo and Peggy Wier, 'Antimerger Policy under the Hart-Scott-Rodino Act: A Reexamination of the Market Power Hypothesis' (1985) 28 JL & Econ 119. The methodology was dubious, and the study made significant errors, including misdating one merger by seven years. See Gregory J Werden and Michael A Williams, 'The Role of Stock Market Studies in Formulating Antitrust Policy Toward Horizontal Mergers' (1989) 28 QJ Bus Econ 3. The *Journal of Political Economy* published a study purporting to show that US price-fixing prosecutions had no effect. Michael F Sproul, 'Antitrust and Prices' (1993) 101 J Political Econ 741. The study relied on unsuitable data and methodology. See Gregory J Werden, 'Assessing the Effects of Antitrust Enforcement in the United States' (2008) 156 De Economist 433, 437. In contrast, very little of the evidence that price fixing significantly raised prices has appeared in highly rated scholarly journals. For a comprehensive review of the evidence, see John M Connor, 'Cartel Overcharges' in James Langenfeld (ed), *Research in Law and Economics*, vol 26, *The Law and Economics of Class Actions* (Emerald 2014) 249.

65 For a detailed account of how the FTC did this in the late 1970s, see William E Kovacic, 'Using Ex Post Evaluations to Improve the Performance of Competition Agencies' (2006) 31 J Corporation Law 503, 524-29.

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even make merger litigation into a battle of merger retrospectives, perhaps giving rise to what journalists termed the '*CSI* effect',[66] as courts demand scientific proof. Any such effects would demonstrate the wisdom of Alexander Pope: 'A little learning is a dangerous thing.'[67]

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66 Research appears mixed on whether the powerful forensic evidence seen on television is demanded by juries to convict. See eg Simon A Cole and Rachel Dioso-Villa, 'Investigating the "*CSI* Effect" Effect: Media and Litigation Crisis in Criminal Law' (2009) 61 Stanford L Rev 1335; NJ Schweltzer and Michael J Saks, 'The *CSI* Effect: Popular Fiction about Forensic Science Affects the Public's Expectations about Real Forensic Science' (2007) 47 Jurimetrics 357; Donald E Shelton, 'Juror Expectations for Scientific Evidence in Criminal Cases: Perceptions and Reality about the "CSI Effect" Myth' (2010) 27 TM Cooley L Rev 1.

67 Alexander Pope, An Essay on Criticism pt 2, l 15 (1711).